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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Formation of hydroxyl radical from San Joaquin Valley particles extracted in a cell-free solution" *by* H. Shen and C. Anastasio

Anonymous Referee #1

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This study analyzes OH formation rates from PM collected at one urban and one rural location in California. It also compares OH formation rates from different size fractions (PM 2.5 vs. PM coarse fraction), and during different seasons (summer vs. winter). OH production is also evaluated in the presence and absence of ascorbic acid, and role of transition metals (Cu and Fe) is identified in that OH generation. The manuscript is well written; detail oriented, well organized, and is recommended for publication after consideration of possible issues raised bellow. The most important issues that need to be clarified and discussed in more detail are: 1. What was the storage time before the samples were analyzed? We can see from the text that the samples were collected during the period between 2006 and 2009, but not when are those samples analyzed.

In terms of similarity (comparison) between PM oxidative activity for samples collected from different locations, seasons, size fractions, this study provides probably reasonable reliable results, but in terms of absolute PM oxidative activity, samples should be analyzed immediately, or at least short after collection. If the samples were not analyzed immediately, then the authors should discuss how big influence will that storage time have on the measured oxidative potential? Did the authors checked and analyzed couple of samples immediately after collection? Answering these questions will help readers in defining how relevant are the results from this study to the oxidative potential of freshly generated ambient particles that will be inhaled by humans. In the other words, the implication of this study to the actual atmospheric aerosol is needed. For example, Chen and Hopke (2010) measured short-lived, highly reactive fraction of the reactive oxygen species (ROS) from the limonene/ozone reaction, as the fraction that was lost when samples were stored for 24 hours in a freezer. That fraction was found to account for up to 17% of the total ROS measured immediately after the collection. How much are the authors expect to loose after some longer storage time (years?)? 2. Type of filters used in this study should be clarified. If quartz fiber filters were used, then the authors should be aware that in general quartz material could generate OH radicals (believed by health community for why the quartz fiber can be harmful for the pulmonary system). That effect can be even higher when quartz is in contact with organic material than in simple blank samples that were used as a control in this study. Thus, a couple of parallel teflon filters should be collected and analyzed together with quartz in order to eliminate that possible positive artifact. 3. The authors did not mention anything about the possibility that the ROS (and it includes OH radicals as well) can be already present on the particles before inhalation or extraction, and not formed only endogenously or via reduction of oxygen species by the reduced forms of transition metals. Significant amounts of ROS were measured in respirable ambient particles prior to inhalation (Venkatachari et al. 2005; 2007), as well as in the secondary organic aerosol (SOA) formed under laboratory conditions. Is it possible that those species can be responsible (at least partially) for high OH production by the Fresno winter 2009

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11, C7627–C7629, 2011

Interactive Comment

(PM 2.5 fraction, normalized to air volume) or Fresno winter 2007 (PMcf, normalized by mass) samples in the absence of ascorbate? 4. Number of samples analyzed for each condition (with asc or without, etc) should be mentioned also in the main text of the manuscript and not only in the text describing Figures, since that is good indicator that more studies should be done in this field to support the presented results. Probably it will be the best to discuss that under “Implications and uncertainties”.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 16861, 2011.